A longer version of Fitch, Hauser & Chomsky (in press, Cognition) was initially accepted at Cognition as a response to Pinker & Jackendoff, but at the request of the editor, J. Mehler, was substantially cut for publication; the shortened version will appear in an issue following Pinker & Jackendoff's target paper. We are posting the Appendix to the longer version here for those interested in our response to their discussion of the Minimalist Program, entirely left out of the shortened version.

Appendix. The Minimalist Program

N. Chomsky, M.D. Hauser & W.T. Fitch

A large part of PJ is devoted to their version of the minimalist program (MP), a topic scarcely mentioned in HCF. However, their account misconstrues the program and its background so seriously that a brief review is warranted.

1. Misunderstandings of MP and its origins

A central concern of the study of language, including generative grammar, is to identify the properties specific to the human faculty of language (FL). Within the biolinguistic perspective -- which began to take shape 50 years ago -- the concern is transmuted into the effort to determine the genetic endowment specific to FL, the (virtually shared) initial state of FL, the topic of "universal grammar" (UG); particular grammars are concerned with attained states, "internal languages" (I-languages). In these terms, methodological considerations can often be reframed as empirical theses concerning more general properties of organic systems, and subjected to comparative analysis.

Two basic conditions that UG must satisfy are that it (1) accommodate the attainable I-languages, and (2) account for their acquisition. Early work in generative grammar revealed enormous apparent diversity of structures and rules within and among languages, indicating that UG must allow for complex descriptive technology, while at the same time restricting the choice of possible I-languages so that condition 2 can be met. The tension seemed irresoluble, and left the study of the evolution of language a remote prospect. A central goal of research has always been to reduce the wealth of postulated descriptive technology by resort to more broadly explanatory principles. There was progress in this project, but it faced a crucial conceptual barrier that was not overcome until the Principles and Parameters approach crystallized in the early 1980s, opening ways to carry the central project forward with greater hope of success. These are the reasons for the 1970's assumptions about "intricate structure of specific rules and guiding principles," to which PJ refer, and for the subsequent progress in dismantling them, which PJ perceive to be a "major recantation" – in fact, more rapid progress in efforts dating back 50 years.

By the mid-1990s, many researchers felt that progress and convergence in this endeavor was sufficient to identify MP as a reasonably integrated research program that focuses on these issues (Brody, 1995; Chomsky, 1995). Later contributions are too numerous to mention, but among

them are Abraham et al. (1996); Boeckx (2003); Brody (2003); Collins (1997); Epstein et al. (1998); Epstein & Hornstein (1999); Epstein & Seely (2002); Hendrick (2003); Hornstein (2001); Lasnik (1999, 2003); Martin et al. (2000).

It has been a useful research guide to formulate the Strong Minimalist Thesis (SMT), which holds that language is a "best possible" solution to the problem of linking SM and CI. In these terms, the task of MP is to clarify the notions that enter into the SMT and to determine how closely the thesis can be approached. Insofar as this can be achieved, the traditional concerns of identifying the specific features of FL are advanced, and the study of its evolution rendered more feasible.

As often stressed, MP is a *program*, not a *theory*; hence the name. PJ regard it as an admission of failure that Lasnik describes MP as a "general program" – as it will always be, by definition. Along the way, however, specific hypotheses may be proposed about the nature of language, and are, as the citations above reveal. Contrary to PJ, there are no "Minimalist hypotheses about the empirical nature of language," and cannot be. Rather, there is a program that pursues what has always been the core issue of the study of language. The SMT in particular is not a "minimalist hypothesis" about language, but a research guide. Note further that MP is independent of one's particular approach to language, over a wide range. One can decide to be interested in the research program or not, to regard it as feasible or premature; but beyond that, no general question of legitimacy arises about the program, whether in the study of FL or any other biological system.

In the few references to these topics in HCF, we suggested that the SMT could indeed be approached in interesting ways: FLN "comprises only the core computational mechanisms of recursion as they appear in narrow syntax and the mapping to the interfaces." Of course this has to be spelled out, a task we did not undertake in HCF, though it is a topic of a great deal of other work (some cited above). Since the "mapping to the interfaces" is explicitly included in this description, it follows that the suggestion in HCF (by definition) includes phonology, formal semantics, the structure of the lexicon (morphology, words), etc., insofar as they are language-specific: in the terminology of HCF, insofar as they belong to FLN rather than FLB. The "core computational mechanisms of recursion" include the indispensable operation *Merge* and the principles it satisfies. To discover these principles has been a central research task in generative grammar since the 1950s. Among them are, for example, locality conditions that have been investigated intensively. A more far-reaching problem is to determine to what extent these principles belong to FL, or are more general factors that interact with genetically-determined FL properties to yield I-languages.

In the early '90s, Chomsky and Lasnik (1993) outlined what seemed to them the best current ideas about the nature of UG, adopting a model similar to what PJ may mean by "GB theory." Since then, a good deal of work has been devoted to investigating stipulated components of that model to determine whether the phenomena

for which they were designed can be derived by keeping more closely to the SMT – or even better, with richer empirical coverage.

One stipulated component of the model that clearly merited investigation was internal levels of representation -- in particular d- and s-structure. An obvious desideratum is to show that no extra levels of representation need be stipulated beyond the two interface levels, hence that d- and s-structure are superfluous. This is the only example of work in MP that PJ mention, but their characterization is inaccurate. Like all postulated linguistic levels, d- and s-structure have stipulated properties that extend descriptive technology and power. These particular stipulations also lead to considerable redundancy in rule systems, with independent cycles of rules that cover much the same ground. By the mid-'90s, it appeared to be possible to eliminate these extra levels and the redundancies they impose, and the results have since been improved. These results relied on such natural conditions of efficient computation as: (1) preserve the syntactic objects merged without modification, thus eliminating extra computation, and (2) minimize phonological computation. Adherence to these conditions yields the so-called "copy theory" of movement, which receives empirical support from the fact that it delivers the elements ("copies") required for interpretation at the CI interface while also, as required on empirical grounds, deleting all but one (with principled exceptions) at the SM interface. As is well known, such reliance on more fundamental principles also improved empirical coverage, strengthening the conclusions even further; thus most of the very productive work of the past ten years on reconstruction effects has relied on this approach (among many others Fox, 2000; Hornstein, 2001).

PJ recognize and apparently accept these conclusions, but, surprisingly, regard them as a failure of the MP. They apparently see no gain in the elimination on principled grounds of the stipulated extra structure and level-specific principles, descriptive technology, and redundant rule systems, while preserving the empirically required structures (with branching nodes, etc.). They also claim that these advances (as we understand them to be) require comparison of derivations and yield syntactic objects with traces and "numerous empty nodes which morphemes are destined to move to or be coindexed with."

The latter claim is correct in only one respect. Early work in MP did suggest that generation involves comparison of derivations, leading to computational complexity in a linear system. The problem was recognized at once within MP research, proposals to resolve it quickly appeared (among others Collins, 1997; Frampton & Guttman, 1999), and variants of them have been generally assumed for years. As for traces and coindexing, work in MP in the early 90's argued that they can be eliminated in favor of the more principled and empirically more adequate copy theory. Elimination of this rich descriptive technology was one of the original goals of MP, in large measure achieved over a decade ago. The only "empty nodes to which morphemes are destined to move" are those for which empirical justification has been

provided (e.g., Cinque, 1999, and the "cartographic project" generally).

With PJ's errors removed, the alleged failure of MP is that reliance on efficient computation yields structures appropriate for mapping to SM and CI interfaces, expanding empirical coverage and explanatory force.

PJ's discussion of the success in eliminating d- and s-structure appears to be based on a misunderstanding of the notion of "linguistic level" as it has been used in generative grammar and in structural linguistics. Take, say, Morris Halle's classic argument 45 years ago that the level of structuralist phonemics introduces rule redundancy, and can be eliminated without descriptive loss. It would not be a counterargument, following PJ's reasoning, to say that his approach still clutters up derivations with distinctive features. Eliminating a postulated level with its stipulated properties and overcoming rule-redundancy, while yielding the structures required at the interface, is a contribution to identifying the properties of FL and removing barriers to the study of language evolution.

PJ also misunderstand the notion of redundancy that has always been used in this context, namely, redundancy of rule systems. They object that language use involves plenty of redundancy. That is indisputable. It is also irrelevant. It is no criticism of Halle's argument that speech contains massive phonetic redundancy. Nor would such a criticism be relevant to other familiar examples, e.g., the concerns of the 1960s that the postulated rules of *raising* and *passive* redundantly generate such constructions as "John was believed to be intelligent." Pursuit of the intuition that rule-redundancy probably indicates error led to the discovery that neither of the postulated rules exists, but only a more general principle Move-NP, later simplified further, and by now plausibly taken to be an application of the indispensable principle Merge when unwanted stipulations on its application are eliminated (Chomsky, 2001). The rather consistent discovery that redundancy of rules indicates error, as in these two familiar cases, was one of the factors that contributed to the belief, by the mid-1990s, that MP can be a productive research program.

PJ regard it as a defect that progress in MP sometimes revives earlier notions, reformulating them in a different framework. That this regularly happens is surely true, as widely discussed. To select a significant case, the elimination of d- and s-structure, along with the redundancy of cyclic rules that it entails, depends on a much simplified variant of the notion "generalized transformation" that was discarded 40 years ago for sound reasons of computational efficiency (which no longer apply in improved theories). This is a criticism only if one regards science as a kind of war, in which ideas that appear in the past or elsewhere must be opposed at all costs.

PJ believe that MP ignores such morphological properties of language as case and agreement. Even a cursory look at the literature reveals that these have been among the topics most intensively investigated within MP; e.g., virtually every source already cited and innumerable others

have been concerned with structural vs. inherent case, long-distance vs. local agreement, intervention effects, multiple-agree and feature-sharing, and numerous similar questions in diverse languages. The same is true of other topics they claim are overlooked: pronouns and anaphora, discourse-related semantics vs. argument structure, scopal issues, topic and focus, etc. – again, central topics in the MP literature, including the sources cited above. PJ also think that phonology is disregarded. That is in principle impossible: the basic goal, repeatedly stated, is to determine the nature of the SM-CI link, therefore including phonology. The same observation is explicit in HCF, as just noted.

In an apparent effort to support their misunderstanding about phonology in MP, PJ cite an "egregious" statement of Chomsky's (p. 118-9, Chomsky, 2000): namely, that "the whole phonological system looks like a huge imperfection, it has every bad property you can think of." This statement has nothing at all to do with the fact that (by definition) phonology is included within MP. The sentences they cite are part of a discussion of an entirely different question, clearly important though still beyond serious research as noted there: namely, the question of how well phonological rules constitute a good solution to the problem of relating narrow-syntactic objects to SM, where "good solution" is to be explicated in terms of principles of computational efficiency that we hope to find exemplified elsewhere in the organic world. Note that the very sentences they cite again take for granted, explicitly, that phonology is included within MP.

Their entire discussion here reveals a complete misunderstanding of the notions "looks like an imperfection" and "apparent imperfection" as used in the source they cite, and the MP literature generally. Thus they write: `Calling [movement] an "imperfection" ignores the fact (which Chomsky elsewhere notes) that movement allows sentences to use some aspects of word order to convey topic and focus while others convey who did what to whom (Chomsky, 2000, p. 13).' First, it is not called an "imperfection" but an apparent imperfection (or "imperfection" in quotes, to stress that it is only apparent): and as is entirely explicit in the source they cite, and throughout, one research task is to overcome the appearance of imperfection by showing that movement operations receive an explanation in terms of the SMT, that is, by reliance on interface conditions and principles of efficient computation. The comment from Chomsky they cite on conveying topic and focus, etc., is part of a discussion about how interface conditions motivate movement operations, and other work that they do not cite brings in principles of computational efficiency, pointing out that movement operations are an optimal device to satisfy these interface conditions because, as noted earlier, they are a special case of Merge that can only be blocked by stipulation, hence violation of computational efficiency (Chomsky, 2001).

2. The relationship of MP to other areas of inquiry

As noted in the body of our response, PJ attribute to Chomsky views about language and communication that he explicitly rejects in the passages they

cite. Extending the fallacious critique we have already discussed, PJ write that MP may be influenced by Chomsky's comments on a "rudimentary wing," which they interpret as the argument "often raised by creationists" that wings (and language) cannot have evolved because part of a wing is not useful for flight. Explicitly and unambiguously, the passage they cite *rejects* this argument; that is the entire point of the passage they cite. The reference to a "rudimentary wing" is followed at once by explicit refutation of the fallacy in the familiar way -- which PJ then go on to repeat. To quote from the very passage they cite, creationist-style fallacies about "rudimentary wings" fail to recognize that "organs develop to serve one purpose, and, when they have reached a certain form in the evolutionary process, become available for different purposes, at which point the processes of natural selection may refine them further for these purposes." The passage gives as an illustration proposals that insect wings initially evolved as thermoregulators (Kingsolver, 1988), and recommends that language should be studied just as other biological systems are (p. 167, Chomsky, 1988).

When we correct PJ's reversal of the explicit and unambiguous wording of the source they cite, we find that there is indeed a connection to creationism: namely, the source they charge with creationist fallacies explicitly *rejects and refutes* them. Of course, the refutation of creationist fallacies has nothing whatsoever to do with MP, as they claim on the basis of serious misrepresentation of their source.

Possibly PJ's consistent misinterpretations are based on rejection of the entire enterprise of the past half century, and traditional grammar before it. We have already cited a number of illustrations. Another indication that this may be so is PJ's puzzlement that "most of the technical accomplishments of the preceding 25 years of research in the Chomskyan paradigm must be torn down" if MP is pursued, and that "investigators, who had significant research commitments in the Government-Binding framework, have abandoned that framework and much of its conceptual inventory, virtually overnight." That would indeed be puzzling for anyone who lacks interest in the core concerns of traditional grammar and generative grammar – namely, to discover the distinguishing properties of language -- and therefore sees no point in abandoning rich descriptive technology that has been argued to be superfluous and misguided. Nothing else is "torn down" or "abandoned" in MP, just as proposals about structuralist phonemics, and transformational and phrase structure rules, and much else, were "torn down" and "abandoned" from the 1950s only when it was shown that their complexity and variety could be reduced to more elementary and far-reaching principles, often leading to improved empirical coverage. These moves have also had the positive effect of excluding impossible structures and enhancing explanatory force – and, accordingly, improving the prospects for study of the evolution of language, always a background consideration from the earliest days.

Perhaps this sample is sufficient to suggest that every statement of PJ's

about MP should be checked against the texts to which they refer; and more generally, the rich literature of the past ten years that they ignore, some cited earlier. Little survives such analysis, as far as we can determine.

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[Note: many of these references are from the longer version of the paper, and thus do not appear in the appendix.]

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